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Alytes as a forum

Alain DUBOIS

Vertébrés: Reptiles et Amphibiens, USM 0602 Taxonomie & Collections, Département Systématique & Evolution, Muséum national d'Histoire naturelle, 25 rue Cuvier, 75005 Paris, France
<adubois@mnhn.fr>

La culture ce n'est pas avoir le cerveau farci de dates, de noms ou de chiffres, c'est la qualité du jugement, l'exigence logique, l'appétit de la preuve, la notion de la complexité des choses et de l'arduité des problèmes. C'est l'habitude du doute, le discernement dans la méfiance, la modestie d'opinion, la patience d'ignorer, la certitude qu'on n'a jamais tout le vrai en partage; c'est avoir l'esprit ferme sans l'avoir rigide, c'est être armé contre le flou et aussi contre la fausse précision, c'est refuser tous les fanatismes et jusqu'à ceux qui s'autorisent de la raison; c'est suspecter les dogmatismes officiels mais sans profit pour les charlatans, c'est révérer le génie mais sans en faire une idole, c'est toujours préférer ce qui est à ce qu'on préférerait qui fût.

Jean ROSTAND, 1963: 47

Scientific publications have gone through a major revolution in the last decades. This revolution consists in a double, contradictory movement: on one side much more freedom for the expression of different ideas, opinions, theories and hypotheses; on the other much less freedom, and the progressive imposition on scientists of a "consensual", "majority", "official" or even "compulsory" discourse.

The first side comes from the development of easy, cheap, accessible to all, international means of communication and "publication" by electronic means, through "sites" on the world wide web. Virtually anybody, in any place of the planet, can create his/her own website, and "publish" his/her own writings, ideas, proposals and projects, so that this system may appear very "democratic" and fair. "Publishing" (i.e., "making public") on the web avoids to have to go through a process of review by referees, and facing the genuine problems associated with this practice (which include censorship of opinions adverse to those of the reviewers, as well as piracy of results and ideas[1]). However, only a few websites (those that appear on top of the address lists provided by most search engines) really have a wide distribution and are largely

1. Contrary to what some seem to believe, this is not an exceptional fact. For example, the next issue of *Alytes* will contain a paper which, submitted elsewhere, had been refused, but was used by a referee to modify (on proofs) a paper that was in press. Hundreds of similar examples could be mentioned, but one of the untold (and rarely transgressed) rules of the current system is to remain silent on such facts.

consulted. Furthermore, most of the websites or pages have only a short life, and are not stored for permanent conservation, so that considering and quoting them as scientific publications is highly questionable, as a reference must be liable to be found and consulted even after decades (Dubois, 2003). Despite projects for a long-term conservation of the contents of web pages as they were at a given date and under a given address (Dellavalle et al., 2003), this is still far from being a common practice, and, for the purpose of scientific knowledge, information and references, most web pages must indeed be considered as "unpublished".

Beside this (apparent) freedom of "publication" of scientific results, theories and hypotheses on the web, a very different situation exists in the world of scientific journals. A distinction has always existed between "major" and "well-known" journals, and "local" or "secondary" ones. But the importance of this distinction has become much stronger in the last decade, especially in Europe, because of the growing weight of so-called "impact factors" (IF) for the "evaluation" of the "quality" of the research produced by individual scientists and research teams. The time is far when the quality of a scientific paper was appreciated by the peers on the basis of its contents, irrespective of the journal where it was published. Nowadays, when you meet colleagues and ask them about their recent scientific publications, a growing proportion of them will mention the titles of the journals where they were published and sometimes their IF, i.e., their "notes" in the "hierarchy" of journals, but not necessarily what they deal with. This system of "notes" may have been influenced by the ideologies of sports and "show business" (with competition, ranking, prices, awards, records, champions, etc), which have had a growing (and highly questionable) impact on the whole of our societies in the recent decades. To have a "visibility" in the international community, as well as for the carriers of professional scientists, the funding of research teams, and ultimately the mere possibility to carry out any given research project, publication in these "highly-ranked" journals has become almost compulsory.

However, publication in such journals requires following very precise (although untold) rules, e.g., dealing with some topics only, following certain ideas currently considered "fashionable" or simply "acceptable", using a special vocabulary and a certain style, especially writing in a short and very condensed way (at the expense of quality and precision of ideas, e.g., banning any expression of doubt and writing only peremptory statements). Some authors, especially from some countries, considered "prominent specialists" in their field, or supported by a lobby, have no difficulty in publishing in these journals, including "worthless or even simply stupid papers" (Hołyński, 2003), whereas others, especially when they happen not to agree with the former ones on some of the "consensual" ideas of the time, are systematically "silenced" in such journals, mostly because their papers are generally submitted to review by members of the first category. Hołyński (2003) provided interesting comments on this question:

"Usual (or at least very frequent) [is the] tendency of editors to automatically assume that in case of disagreement between the author and referee it is always the latter who is right and the former must exactly follow the (...) 'mandatory recommendations'. It is difficult to find out what such assumption could be based on. For many years I (for instance), according to my personal (arguably not identical to those of anybody else – including the reviewer) interests and abilities, do the research on particular type of problems: make thousands of observations, read hundreds of papers, think over innumerable theoretical or methodological questions, discuss doubtful points with colleagues; as some more narrowly delimited topic emerges from the background of so gathered general experience, I spend further months or years on its elaboration, and then at least weeks on formulation of text, ensuring its factual, interpretational and formal accuracy, implementing and cross-checking innumerable corrections, 'polishing' the style, etc., until it precisely reflects my interpretation of the results. Then my paper is sent to somebody who – having in most cases faced the subject for the first time in his/her life – will read the manuscript through on a bus, give the matter his/her careful consideration standing under the shower next morning, and... I receive the review from the editor together with the kind information that 'unfortunately the paper has been rejected because of negative opinion of the reviewer' or (in the 'better' case) 'please correct the paper according to the reviewer's suggestions and send it back within two weeks' – the question of whether I agree with the 'suggestions' or not is apparently not interesting to anybody... Somewhat later I am asked to review someone's – perhaps just my earlier referee's – paper and now... *my* opinions are decisive! I do not believe this system to make very much sense! (...) even the most honest and careful referee is not likely to be more conversant *with the particular problems* than the author of the reviewed paper, so there is no reason to assume a priori that his/her critical remarks are valid. And indeed, innumerable examples provided by the history of science show, how frequently even the most respectable authorities are wrong as 'referees', and how destructive can such unjust opinion be (...)".

From a simple scientific point of view, the function of having manuscripts refereed before publication may be very useful to limit *factual* mistakes in some papers: wrong calculations, objective methodological mistakes, unwarranted conclusions drawn from the data presented, etc. Such *objective* criticisms by referees, as defined by BOUR & DUBOIS (1994), can avoid many problems, and even *subjective* criticisms often allow to improve papers: but the difference between both kinds of comments is that the latter should only be considered *suggestions*, not requirements that the author is obliged to follow for acceptance of the paper. Let us come back to HOŁYŃSKI's (2003) words: "I have no objection against – indeed, I like very much – discussions on 'my' topics (...) with anybody interested, but *only* on the condition that: (1) the *last word is mine* (*I* will sign the paper with *my* name, so it must reflect *my* views); (2) the discussion-partner *does not feel offended* if I do not agree with – and consequently do not accept – his/her views; and (3) the exchange of opinions *does not* significantly *delay* the publication: these points are, in my opinion, much more important than possible discovery by the reviewer of some minor mistakes or inaccuracies". Although peer-review is often presented as essential to assure high standard of publications, it is in fact in this rôle only partially efficient, being also powerful as suppressor of valuable but unorthodox and/or "unfashionable" works. Its major function seems to be to eliminate many manuscripts submitted to some journals which, being considered more important than others because of the IF dictatorship, are much more solicited and cannot publish a large proportion of the papers they receive. Another function is clearly to avoid dissident opinions from those of the "leading specialists" in a scientific field.

Recently, in a poorly refereed paper (as it contains several gross factual mistakes)[2], HILLIS (2006) made a plea for "Google taxonomy": he suggested that taxonomists should keep their classifications and nomenclatures unchanged in order to follow the "taxonomic" information provided in major sites on the web: if such a suggestion was to be followed in all domains of science, then we should better stop all scientific research worldwide, as science always produces new results which challenge the ideas of the past. Google and other similar sites should be at the service of customers to find information, including information on the progress of science and changes in the ideas of the past. Otherwise, they will act as a brake against scientific progress.

At every epoch, "leading specialists" have had very strong opinions and they have been angry at those who did not share them. Fortunately, they did not always succeed in "silencing" them. The literature on the systematics and evolution of amphibians is rich in examples of such situations. Let us reconsider a few of them. The North American leopard frogs were long referred to a single species, *Rana pipiens*, which was considered to be very variable according to the regions. This variation was seen as entirely adaptive to climatic conditions, and this example was long given as a good empirical support for the prevalence of gradualistic evolution in zoology. This "model of Moore" was challenged by the discovery that different call types corresponded to different morphotypes, then later to different protein electromorphs, and finally to different species, but it took some time to publish these findings, as they were against the "dogma" that could be found in any textbook on evolution (DUBOIS, 1977). Similarly, all European green frogs were long considered to belong in a single species, *Rana esculenta*, or two species, adding *Rana ridibunda*, but other phenotypes (including that now known as *Rana lessonae*) were considered to be mere variations, or at best subspecies, of the former. When Leszek Berger obtained very strange results in some crosses involving these frogs, he could not explain them but he wanted at least to

2. As this paper was a reply to a paper of mine (DUBOIS, 2006) but contained many confusions and misleading statements, I immediately submitted a rebuttal to the journal, where it was rejected, not because it contained factual mistakes, but for the following reason: "The manuscript is mostly about nomenclature, and as such I feel it is not entirely appropriate for the journal *Molecular Phylogenetics & Evolution*. (...) While I agree that differences and misunderstandings surrounding 'The Code' and 'Phylocode' can cause confusion and misunderstanding in classification and taxonomy, I also feel that debates regarding these differences are better suited to nomenclature journals." (18 September 2006). Nobody knows what are these so-called "nomenclature journals", but anyway my reply had to be resubmitted elsewhere (DUBOIS submitted), and readers of *MPE* will continue to have misleading information about some basic Rules and concepts of zoological nomenclature.

publish his careful observations. He had to wait for several years to publish them, because all editors, professors and specialists would tell him: your results cannot be right, just look at any textbook of genetics. It later turned out that Leszek's observations were correct and the textbooks wrong, because at that time no one knew hybridogenesis and kleptons (DUBOIS, 1977; DUBOIS & GÜNTHER, 1982; GRAF & POLLS PELAZ, 1989). At the times of these two stories, the system of referees was not prevalent, and most of the decisions regarding acceptance or refusal of papers were in the hands of the chief editors of the journals, but it is likely that the referee system would have produced similar results: except in some noteworthy cases, the referees express the "consensual opinion" of the scientific community in which they work, and they are shocked or afraid by papers that do not follow the general trend. This is the very essence of the system, and it is uncertain whether the works of Galileus, Wegener or Hennig would have been published if they had been submitted to "peer-review", especially by "prominent specialists" of their disciplines.

A scientist may be very good, careful, brilliant, he may be right in many cases, but he may also happen to be wrong, as no one is infallible: this is why the "argument of authority" ("it must be so, because the great specialist Mr So-and-So thought it is so") is not a scientific argument (just like the "proof by Google"). Let us consider just George Albert Boulenger, certainly one of the best amphibian taxonomists ever (considering the concepts and techniques available at his time). A large majority of the species and other taxa he described as new are still considered valid today, and many of his opinions in controversial cases were later supported. Many, but not all. He thus debated with Nelson Annandale (ANNANDALE, 1917; BOULENGER & ANNANDALE, 1918; BOULENGER, 1920) on the status of the Indian frog then known as *Rana crassa* (now *Hoplobatrachus crassus*), which Boulenger considered a "variety" of *Rana tigrina* (now *Hoplobatrachus tigerinus*), whereas Annandale, who had observed both forms in life, considered them as distinct species. The debate between the two men ended with a peremptory statement of Boulenger that he was certainly right, as this case was similar to that of *Rana esculenta* and *Rana lessonae*, which he regarded as mere "varieties" of a single species: it turned out that in both cases Boulenger was wrong, and that Annandale's opinion on the specific status of the two Indian forms was correct (DUBOIS, 1974; KOSUCH et al., 2001; GROSJEAN et al., 2004). Another case where Boulenger turned out to be wrong, also in this case because he was above all a laboratory man, is his refusal to recognize the tree-frog of southern France as a distinct species from that of northern Europe, although Louis-François Héron-Royer, an excellent field batrachologist, had described it as *Hyla barytonus*, using for the first time the criterion of male calls to distinguish two morphologically very similar frog species (HÉRON-ROYER, 1884; Boulenger, 1898): today, since the work of PAILLETTE (1967) on mating calls, the species status of the southern form (now known as *Hyla meridionalis*) is accepted by all.

Innumerable examples of this kind could be given, coming from all branches of science. In many cases, after some time, mistakes have been corrected, and which was once a minority opinion is now firmly established. In some cases, because some voices were silenced, some results ignored or censored, this "normal process" of correction of mistakes has not yet occurred – perhaps it will never occur. The consequences are not always dramatic for science and for mankind, of course. Cases like the Lysenko-Michurin years under Stalin, or so-called scientific support from some biologists for the racist nazi theories, are fortunately rare in history. But they may always come back. At any rate, science has never anything to gain to censorship, to silencing the opinions of those who do not think like the majority, or more exactly like those who control the sources of power in the scientific community (who are not always the majority). The arrogant attitude of some referees and editors of scientific journals nowadays is not acceptable, and should not be accepted by the scientific community. We do not need ayatollahs who "possess the truth" in science, they are numerous enough in the rest of our society.

The repeated efforts of the journal *Alytes* to be indexed in the *Current Contents* and the ISI database (which provides the impact factors) having failed until now, and the journal having no sponsor or institutional support of any kind, its long-term survival is highly uncertain. The journal is published by a non-profit society, and it lives only on the support of its subscribers, readers and authors, including through page charges and occasional gifts (which are always welcome). Anyway, as long as the journal will exist, the hope of its founder is that it will remain, as it has been from the start, open to different opinions, different approaches, different kinds of works and ideas. To make this even clearer, in this issue we start a new section of the journal, entitled *Forum*. All interested colleagues are welcome to send us papers raising unusual questions, proposing unorthodox approaches or opinions, or presenting strange, unexplained findings or results dealing with amphibians, amphibian biology or more general questions if these apply to amphibians (as is the case in this issue). Readers are welcome to reply, as long as they remain within the limits of an intellectually honest debate among colleagues, with mutual respect between contradictors. No censorship will be exerted on papers submitted to this section of the journal, although factual mistakes or clear methodological flaws, if detected, will of course not be published.

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